The neglect of experiment

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Introduction

One of the great anticlimaxes in all of literature occurs at the end of Shakespeare's *Hamlet*. On a stage strewn with noble and heroic corpses – Hamlet, Laertes, Claudius, and Gertrude – the ambassadors from England arrive and announce that "Rosencrantz and Guildenstern are dead." No one cares. A similar reaction might be produced among a group of physicists,[1] or even among historians and philosophers of science, were someone to announce that "Lummer and Pringsheim are dead." And yet they performed some of the most important experiments in the history of modern physics. It was their work on the spectrum of black-body radiation,[2] along with that of Rubens and Kurlbaum,[3] that showed deviations from Wien's Law and formed an important part of the background to Planck's introduction of quantization.

This is symptomatic of the general neglect of experiment and the dominance of theory in the literature on the history and philosophy of science. In Thomas Kuhn's history of quantization, *Black-Body Theory and the Quantum Discontinuity, 1894–1912*,[4] Lummer, Pringsheim, Rubens, and Kurlbaum are, at best, peripheral characters. The title indicates what Kuhn thinks is important. We never see what the experimental results were or find a discussion of how they were obtained.

But, it might be said, that is only an isolated case. Surely everyone is aware of the famous experiments of Galileo and the Leaning Tower of Pisa, of Thomas Young's double-slit interference experiment, and of the Michelson–Morley experiment. What seems to be generally known, particularly by scientists, about these experiments shows the mythic treatment of experiment. Real experiments and their roles are not often dealt with.

According to the myth, Galileo dropped two unequal weights from the top of the tower, observed that they fell at equal rates, and thereby refuted Aristotelian mechanics and established the importance of experiment in physics. There are several problems with this story. First, there is serious doubt that Galileo ever performed the experiment.[5] Had he done so, he would have gotten rather confusing results. A modern replication of the experiment showed that in a fall of 200 feet, a shotput will beat a softball by 20 to 30 feet.[6] This is not a large enough difference to satisfy the Aristotelian theory, but it is too large for equality of fall, or the hand's-breadth difference Galileo reported. Even had Galileo done the experiment and observed his reported results, an Aristotelian could easily have modified the theory to accommodate the data.[7]

In the case of Young's experiments, John Worrall has argued that, contrary to popular belief, these experiments did not decisively refute the corpuscular theory of light and establish the wave theory.[8] He points out that corpuscular explanations of both interference and diffraction were available. In addition, until Fresnel's later work, the wave theory could not explain the rectilinear propagation of light, which was at least as serious a problem for it as interference was for the corpuscular model.

Similarly, the Michelson–Morley experiment was supposed to have demonstrated the nonexistence of the ether and to lead directly to Einstein's special theory of relativity. Although Einstein's 1905 paper on relativity did mention the failure of the then recent attempts to measure the velocity of the earth relative to the ether, no specific mention was made of the work of Michelson and Morley. Historians of science have disagreed on the importance of this work for Einstein's theory, but in no case do they assert the kind of importance it has been given in more popular accounts such as textbooks. This error can, of course, be attributed to physicists' lack of knowledge of the history of their discipline, and this is partially true, but similar accounts appear in philosophical discussions.[9] Even a detailed historical study of the Michelson–Morley experiment[10] failed to point out that in their 1887 paper there was an important difference between the raw data given in the tables, which showed a large linear drift, and the graph of residuals, which gave the well-known null result. Actually, Michelson and Morley set an upper limit for the velocity of the earth relative to the ether: one-sixth of the earth's orbital velocity. An excellent study of this has been published by Mark

Handschy,[11] who has given a plausible reconstruction of the method used to subtract the drift. The point here is that real, as opposed to mythological, experiments are rarely discussed, even when experiment is mentioned at all.

Fortunately, recent work on actual experiments seems to be reversing this trend, or at least modifying it. On the philosophical side there is Ian Hacking's excellent book *Representing and Intervening*,[12] which argues persuasively, using numerous examples and illustrations from the practice of science, against the idea of theory-dominated experiment and in favor of the view that experiment often has a life of its own. Historians of science such as Peter Galison, Andrew Pickering, David Gooding, John Worrall, Bruce Wheaton, and Roger Stuewer have presented detailed accounts of various experimental episodes.[13]

This book is intended as a contribution to this continuing study of the history and philosophy of experiment. It will deal primarily with two questions.

1. What role does, and should, experiment play in the choice between competing theories or hypotheses or in the confirmation and support of theories or hypotheses?
2. How do we come to believe rationally in the results of an experiment, or how do we separate a result, obtained by use of an apparatus to measure or observe a quantity, from an artifact created by the experimental apparatus?

In answering the first question, philosophers of science, with the exceptions of Popper, Glymour, Hacking, and Shapere,[14] seem either to undervalue the role of experiment, where they acknowledge it at all, or to take observational or experimental results as given and unproblematical. The philosophical positions range from what one might call the sociological and psychological views of Feyerabend and Kuhn to the logical problems raised by Duhem and Quine.

In *Against Method*,[15] Feyerabend argues for methodological anarchy in science. He attributes scientific change to propaganda victories by one group of scientists over another and allows no role for experiment in determining the decision between two competing theories. Kuhn, in *The Structure of Scientific Revolutions*,[16] argues that major scientific change occurs by paradigm shift. He regards two competing paradigms as incommensurable, and although he does allow for rational discussion and experimental evidence as parts of the decision-making process, he does not seem to regard them as major components. He states that "The competition between paradigms is not the sort of battle that can be resolved by proofs."[17] In Kuhn's view, there can be no falsifying instances or crucial experiments.

A similar position is taken by Lakatos, who argues that a theory can be rejected on the basis of experimental evidence only if an alternative and better theory is available: "... no experiment, experimental report, observation statement or well-corroborated low level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory."[18]

Duhem and Quine[19] have raised a logical objection to the role that experiment plays in the refutation of hypotheses. They have argued that any theory or hypothesis can be saved from refutation by some suitable adjustment in background knowledge or by auxiliary hypotheses. Quine states that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system."[20] If this is so, then only the whole of science can be affected by experimental evidence.

Recent work on the confirmation of scientific theories or hypotheses tends to regard observations or experimental results as given,[21] although Glymour[22] does offer historical examples to support his bootstrap model of confirmation.

The second question regarding the epistemology of experiment has been almost totally neglected by philosophers of science, except for the recent work of Ian Hacking and Dudley Shapere referred to earlier.

The approach taken in this study will be to combine detailed historical study of episodes in the history of twentieth-century physics with a discussion of some of the philosophical issues. These episodes will include (1) the experiments on parity nonconservation, both in 1957, when the violation was discovered, and in 1930, when it could have been but wasn't, (2) the discovery and acceptance of CP violation, and (3) Millikan's oil-drop experiments, which established the quantization of electric charge and also measured e, the fundamental unit of charge. As I admit in Chapter 4, I do not have a general answer to the first question concerning the role of experiment in theory choice. These episodes do, however, provide examples of "crucial" and "convincing" experiments that played major roles in theory choice. I shall also argue that this role can be philosophically justified in these particular episodes. This provides at least some counterexamples to those philosophers and sociologists of science, some of them discussed briefly earlier, who would deny that role.

That role demands that we have good reasons for believing in experimental results. The second half of this book is devoted to discussing how we can come to believe rationally in these results. This, too, has been questioned. In Chapter 6 I shall present a set of strategies, used by practicing scientists (and which I argue can be independently justified), that provide us with rational grounds for belief. Here, too, the emphasis will be on the actual practice of science, and the three episodes mentioned earlier will provide some of the evidence. I shall also discuss two other questions concerning experiment: the possible undue influence of theoretical preconceptions, and the question of fraud in science.

This study is certainly not a complete discussion of experiment in physics. I have concentrated on only one of the many roles that experiment plays: that involved in theory choice or the confirmation of theories. In Chapter 4 we shall discuss some of the other roles it plays.

A few words here on the methodology I have used: I work primarily with what philosophers of science have called the context of justification, and consequently with published papers, the artifacts that scientists have submitted for peer review and as their contributions to the permanent record. This is not to say that publication is the sole means of communication among scientists. Preprint circulation, correspondence, conversation, and attendance at meetings and conferences are also important methods of information exchange. Publications also do not give a complete picture of an experiment. Laboratory notebooks, correspondence, and interviews give us more information, and the laboratory notebooks will be used in two of the episodes to be discussed. I do believe, however, that for the questions this study will consider–theory choice or confirmation and the validation of experimental results – the published record should be used. I think that the information acquired by an experimenter, by any means, is essentially that contained in the published work, and I think that the published reasons given both for the motivation of the experiment and for the acceptance or rejection of hypotheses are those that in fact determined the course of the work. Whatever an experimenter's private reasons for believing in a result, I think that only those that the author is willing to state publicly should be considered in discussing the validity of those results.